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VAT Cuts as Emergency Policy Intervention: Evidence from the UK Case *

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Abstract

In July 2020, the UK government reduced the VAT rate on hospitality services from 20% to 5% as an emergency policy intervention. Using a novel dataset of detailed hotel room characteristics in UK and elsewhere, we estimate how much the tax cut was passed on to consumers via a price reduction. We find a statistically significant contemporaneous pass-through to hotel room prices that varies between around 20% and 50%, with a peak effect on prices observed on the second week after the reform. However, the pass-through effect is the outcome of a discretionary approach as discounts were negligible for rooms sold two months after the policy introduction.

JEL Classification: H22, H25, L83.

Keywords: VAT cut, pass-through, hospitality.

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1 Introduction

On the 15th of July 2020, the UK Government introduced a temporary VAT cut in the hospitality sector from the base rate of 20 to 5 percent.[1] This policy was an urgent response to the pandemic, aimed at supporting an industry severely affected by lockdowns and other social distancing measures. The efficacy of these emergency and targeted interventions remains a particularly contentious issue and there is still much uncertainty about their empirical effects (e.g., Crossley et al., 2014; Benzarti et al., 2024). We exploit granular online information about hotel room characteristics around the policy change implemented in the UK, to shed light on a fundamental aspect of this debate: Was this tax cut passed on to consumers via a reduction in hotel prices? The answer to this research question is of first-order policy importance. First of all, various countries have adopted isomorphic industry-specific VAT cuts in response to the pandemic.[2] Moreover, similar policies have either been introduced or actively debated as a response to the most recent inflation crisis.[3]

In conducting our analysis, we adopt a quasi-experimental approach. Specifically, we first collect comprehensive online data from *Booking.com* about prices for different hotel room variants (e.g., single/double room) and other hotel characteristics (e.g., star classification) for premises located in the UK, as well as in other European countries, i.e., Denmark, France, and Italy. Relative to the UK, these countries did not experience a VAT cut for hospitality services but, crucially, had similar social distancing measures in place during summer 2020. Specifically, most of the restrictions were lifted in the four countries examined in this paper at the end of June 2020, boosting tourism over that summer.[4] Overall, our novel dataset comprises 745,892 room variants (readily comparable across countries) over 3,038 hotels. This enables us to implement a very fine-grained assessment of the pricing decisions following the UK VAT rate cut. Then, we employ a difference-in-differences (hence, DiD) technique using the UK as the treated country and Denmark, France, and Italy as the controls. This is our main identification strategy. We fix check-in dates from the 19th up to the 29th of July 2020

[1] The VAT rate was raised to 12.5 percent from the 1st of October 2021, with the standard rate of 20 percent due to return from the 1st of April 2022.

[2] The list of countries that implemented VAT reductions in response to the pandemic includes Austria, Bulgaria, Czech Republic, Germany, Ireland, Kenya, Montenegro, Norway, Paraguay, Spain, Togo, Turkey, and United Kingdom .

[3] Several countries (i.e. Cyprus, Ireland, Netherlands, Portugal, Spain) significantly reduced (in some cases up to 0 percent) the VAT rate on food and/or energy products.

[4] According to the Eurostat data in July 2020, the number of nights spent in hotels increased by 466, 154, and 259 percent, relative to the previous month, in Denmark, France and Italy, respectively (https://ec.europa.eu/eurostat/databrowser/Tourism). The UK experienced a similar increase in tourism. The ONS data, for example, report an increase of 110 percent in the occupancy rates in the accommodation businesses in England during that summer (https://www.ons.gov.uk/businessindustryandtrade/tourismindustry).

and from the 19th to the 29th of September 2020. Finally, we collect hotel room prices for those check-in dates from the 9th (the day following the policy announcement) till the 28th of July and then from the 6th until the 28th of September. The analysis of the prices collected in July, for stays in July and September, allows us to evaluate the immediate effects of the UK VAT cut. Furthermore, by looking at the prices collected in September our approach provides insights on how the hotels responded to the policy in the medium term.

Our results are described as follows. For hotel prices posted in July for check-ins in the same month, we estimate a robust and statistically significant pass-through from the VAT cut that varies between around 20 and 50 percent. We show that its peak effect is in the second week after the reform, i.e., prices observed from the 22nd of July, for July check-ins. Differently, the impact of the VAT cut on the UK hotel room prices for September check-ins appears weaker and often not statistically significant, irrespective of whether these prices were observed in July or September. Our estimates suggest that, even if the policy covered all room/day combinations, the hotel managers seem to have applied in July a discretionary approach by choosing to reduce the prices for stays in the same month, leaving unaltered those for stays in September. Nicolini et al. (2023) also document that French hotels adjusted their prices in a bespoke way in response to an exogenous demand shock. In a nutshell, the main take-home of our analysis is that the effect of the targeted VAT policy on consumer prices of hotel rooms, although substantial, is incomplete in magnitude and limited to certain products because hotel managers discretionally discounted fares based on the stay dates. As we will describe in details, these conclusions are robust to the use of different control groups, the inclusion of several heterogeneous effects in the econometrics model, controlling for parallel trends and no-anticipation, and alternative estimation approaches.

Our analysis contributes to three strands of the literature. Firstly, we relate to the studies on the effects and pass-through of targeted VAT cuts. On this, Harju et al. (2018) estimate the price responses to large restaurants VAT rate reductions in Finland and Sweden and find that the pass-through was almost full for restaurants belonging to chains, while it was nearly zero for independent restaurants. Benzarti and Carloni (2019) evaluate the incidence of a large cut in VATs for French sit-down restaurants in 2009 and find a small yet statistically significant pass-through to consumer prices, with most of the estimated gains from the tax cut going to restaurant owners. Focusing on two targeted tax reforms on Finnish hairdressing services, Benzarti et al. (2020) show that the pass-through is substantially higher when the intervention involves a VAT hike rather than a cut.[5]

[5]Examining a series of increases in the excise duty on petroleum products implemented by the Greek government in 2010, Genakos and Pagliero (2022) show that the pass-though also varies with the degree of competition.

We extend this literature by studying the price pass-through of targeted VAT cuts on a previously unexplored set of products, i.e., the hotel rooms. This is particularly convenient for the aim of the paper. First, prices for this class of products can be easily accessed through online international travel agencies websites. This allows to gather a large number of daily observations that makes our estimates very precise and enables us to quantify, for example, the transitory nature of the VAT cut under consideration. Second, while hotel room packages present a large variety of observable characteristics, these are homogeneous across hotels and countries, e.g., star classification, free cancellation, ratings etc. As such, we can conduct an exhaustive analysis of the policy reform examined here. For instance, we find that whether or not a hotel belongs to a chain makes no statistical difference on its price pass-through.

Secondly, we relate to the literature analysing the effects of emergency fiscal policy during the pandemic. On this, Montag et al. (2020) analyse the emergency VAT cut introduced in Germany in spring 2020 and show that its pass-through on fuel prices was notable but incomplete. Analysing the same emergency reform, Fuest et al. (2021) find that the VAT rate cut was also only partially passed on to the German supermarket retail prices. Bachmann et al. (2021) exploit the temporary nature of the VAT rate cut in Germany (the measure lasted until the 31st of December 2020) to study the response of spending to this emergency policy and find that the tax cut led to a relative increase in durable spending of 36 percent for individuals with a high perceived pass-through.[6]

Relative to this literature we make two contributions. First, we analyse this type of emergency policies for a different country, the UK. Interestingly, like for the German experience, we find positive yet incomplete effects on price pass-through. Second, rather than on an across-the-board VAT cut like the one implemented in Germany, we study the effects of an emergency policy on a targeted industry particularly affected by lockdowns and other COVID-19 restrictions.

In more general terms, we also relate to the studies on emergency and temporary VAT cuts implemented by governments during economic downturns or periods of high inflation. On this, for example, Crossley et al. (2014) found that the initial pass-through caused by the 2008 VAT cut in the UK was partly reversed after only a few months. Also the related increase in spending was small and temporary. Benzarti et al. (2024) examine the temporary VAT cut on basic food necessities introduced as anti-inflationary measure in Argentina in 2022. They find that in the absence of anti-profiteering measures, the prices responded less to the VAT cut than its repeal, resulting in post-VAT cut prices that were higher than their

[6] Dergiades et al. (2023) focus on alternative forms of economic support such as income support and debt/contract relief for households and test their effectiveness in relationship with non-pharmaceutical containment interventions.

pre-VAT cut levels. On this, also our estimated discretionary pass-through of the UK VAT cut casts serious doubts on the efficacy of this type of targeted emergency policies for reducing consumer prices.

The paper is organized as follows: Sections 2 describes the empirical strategy, Section 3 presents the results, Section 4 concludes.

2 Empirical Strategy

Data Description. We base our empirical analysis on scraped daily data on hotel rooms from *Booking.com* (e.g., Lacetera et al., 2021; Mantovani et al., 2021). The daily data collection is divided in two periods. The first one starts on the 9th of July 2020 (that is the day after the fiscal policy announcement) and continues until the 28th of July 2020. The second period begins on the 6th of September 2020 and ends on the 28th of September 2020. Our sample covers 3,038 hotels located in Denmark, France, Italy and the UK.[7] All countries considered ended the first lockdown before the 8th of July 2020 and were comparable in terms of infection rates at the time of the fiscal policy announcement.[8] The ease of the restrictions boosted tourism over the summer. On this, Figure 1 shows the significant increase in the number of nights spent in hotels in Denmark, France and Italy by domestic and international tourists. In line with the aggregate EU-27 figures, these numbers, despite smaller in scale, reflect similar seasonal dynamics to the ones recorded for the same countries in the previous year.[9] Similarly, as shown in Figure 2, the rate of occupancy in English accommodation businesses significantly increased after the end of the first lockdown. Furthermore, none of the countries in the control group (Denmark, France and Italy) experienced a VAT policy intervention during summer 2020.[10]

We use the hotel's *url* code in order to access the full set of hotel rooms on offer. This allows us to identify several possible room variants in terms of characteristics and add-ons, see Figure A.1 for an example. Each room variant corresponds to a unique set of characteristics, e.g., hotel name, room type, add-ons, check-in date, which we combined to create our panel identifiers. For instance, a double room can be sold with or without free cancellation and

[7]Specifically, for the UK we collected data for hotels located in London and Edinburgh, while for the other countries we use hotels located in the followig cities: Copenhagen, Paris, Rome, Milan, Turin, Naples, Florence, Genova, Palermo, Cagliari and Bologna.

[8]According to the Johns Hopkins University CSSE COVID-19 Data, on the 8th of July 2020 the daily new confirmed COVID-19 cases per million people (7-day rolling average) were less than 8 in all the countries in our sample. The numbers remained relatively low ($\leq$ 15) until the end of July 2020 (https://ourworldindata.org/coronavirus).

[9]The data are from Eurostat (https://ec.europa.eu/eurostat/databrowser/Tourism).

[10]The data are from ONS (https://www.ons.gov.uk/businessindustryandtrade/tourismindustry).

breakfast; it could also be sold for single use.[11] The richness of information allows us to identify 745,892 unique products, i.e., room variants. The price of each product v is then used to create the following index, i.e.,

$$I_v^d = \frac{P_v^d}{P_v^0} \times 100, \tag{1}$$

where P_v^d is the price of a product (room variant) v, retrieved on date d. The base date $d = 0$ corresponds to the first day before the policy introduction in which we observed the price for product v. The same was then tracked daily until its check-in day. Using price indices enables us to compare the evolution of prices denominated in different currencies and eliminates possible exchange rate distortions.

Econometric approach. We estimate the impact of the VAT cut on the UK hotel prices by adopting DiD regressions with products located in the UK as 'treated' and those located in Denmark, France, and Italy as the 'control group'. Our baseline regression reads as

$$I_v^d = \alpha_v + \sum_{i=1}^{3} \beta_i \; Post_i + \gamma \; Treated_v + \sum_{i=1}^{3} \delta_i \; Post_i{*}Treated_v + \theta T_{vd} + u_{vd}, \tag{2}$$

where $Post_i$ is a set of dummy variables switching on if the price of product v was retrieved between the 15$^{\text{th}}$ and 21$^{\text{st}}$ of July ($Post_1$), between the 22$^{\text{nd}}$ and 28$^{\text{th}}$ of July ($Post_2$), or after September the 5$^{\text{th}}$ ($Post_3$), respectively. The omitted dummy variable $Post_0$ identifies the period before the 15$^{\text{th}}$ of July, that is our base category. The use of different time categories is aimed at capturing possible differential hotel managers' responses and related price dynamics induced by the policy intervention. Then, the variable $Treated_v$ is a dummy that takes value 1 if product v is located in the UK, and 0 otherwise.[12] Finally, T_{vd} denotes the number of days (in logs) separating the observation of the price of a product and its check-in date. This variable is meant to capture possible price trends that are typically found in applied works on hotel room prices, e.g., increases in prices as the check-in date approaches (Melis and Piga, 2017).

Our main parameters of interest are the δ_i, i.e., the differences between the average percentage price variation before and after the policy introduction, for hotel room variants (products v) in the UK relative to the corresponding price changes in the control group

[11] Furthermore, we also scraped additional characteristics of the hotels such as stars, users' ratings, and chain affiliation.

[12] Since v denotes a set of fixed effects that include the hotel's identifier, the variable '$Treated$' is not independently identified in the regressions.

countries, holding the query period fixed. That is, the estimated parameters $\hat{\delta}_i$ capture the DiD effect of the legislative intervention in the short-term period ($i = 1$ and $i = 2$) and after the policy operated for almost two months ($i = 3$).[13]

As described above, our baseline regression (2) captures the average effects of the fiscal policy reform. However, our analysis needs to take into consideration specific characteristics that are likely to affect the hotels' propensity to react to tax cuts differently. To do this, we extend our baseline regression controlling for the following three general observable characteristics.

First, we control for the presence of bookings with breakfast or free cancellation. Due to social distancing, offering breakfast involved complex organisational procedures. Likewise, the uncertainty about COVID-19 policies could create a stronger incentive to offer free cancellation. Thus, prices of products with these add-ons may have reacted differently to the VAT rate cut.

Second, we control for possible differences in management quality, a potentially important aspect in the price pass-through following the VAT cut. We proxy management quality with various hotel rating measures (e.g., Lacetera et al., 2021). Specifically, we define a hotel as having high (low) management quality if it scores above (below) the median value of its star group in the following four rating categories, i.e., 'cleanliness', 'comfort', 'staff quality', and 'facilities'. Moreover we also control for the overall customer satisfaction. As for the previous measure, we define a hotel as having high (low) overall satisfaction if it scores above (below) the median value of its star group in the 'overall satisfaction' rating as recorded in *Booking.com*. This latter measure is not directly related to how well the hotel is managed (although it certainly correlates with it), but could still contain important information about a hotel pricing decision following the policy change.

Lastly, we distinguish hotels based on whether or not they are affiliated with a chain. This has been found to be pivotal in order to understand price pass-through of targeted reforms similar to the one analysed here (e.g., Harju et al., 2018). Moreover, chain's affiliation is often considered to be a relevant proxy for hotel's economic performance and price structure (e.g., Kosova et al., 2013; Hollenbeck, 2017; Mantovani et al., 2021).

Operationally, we run five separate regressions, controlling for one characteristic at a time. Specifically, these are: 1) bookings with free cancellation; 2) bookings with breakfast included; 3) bookings in hotels with high management quality; 4) bookings in hotels with high overall satisfaction; and 5) bookings in hotels that are affiliated with a chain. For each regression, the dummy variable *Group* takes value 1 if the room variant v belongs to one of

[13] In this sense, our modelling approach follows closely the methods proposed in Wooldridge (2021) to deal with staggered and heterogeneous effects in DiD regressions.

the five above-listed categories and 0 otherwise. The same dummy is also interacted with the *Treated* and *Post*. For each regression, the triple interaction model specification reads as

$$\begin{aligned} I_v^d =& \alpha_v + \sum_{i=1}^{3} \beta_i \; Post_i + \gamma \; Treated_v + \sum_{i=1}^{3} \delta_i \; Post_i{*}Treated_v + \\ &+ \epsilon \; Group_v + \sum_{i=1}^{3} \zeta_i \; Post_i{*}Group_v + \eta \; Treated_v{*}Group_v + \\ &+ \sum_{i=1}^{3} \lambda_i \; Post_i{*}Treated_v{*}Group_v + \theta \; T_{vd} + u_{vd}, \end{aligned} \tag{3}$$

whereas the vector of parameters λ_i captures the effects of the VAT cut in the UK on the price of a product v belonging to one of the five categories listed above.

All the econometric models are estimated separately by using distinct samples based on check- in dates, i.e., 19^{th}- 29^{th} July and 19^{th}- 29^{th}September. In this way, we control for the fact that the July check-in dates only consider prices posted within a short planning horizon of three weeks at most, unlike the September check-in dates that include prices retrieved up to eighty weeks before the check-in. Moreover we use only hotels with star classification between 3 and 5, and run separate regressions for each of them. These star categories comprise the majority of hotels in the countries analysed. Furthermore, hotels in lower star categories tend to be small family-run businesses, generally adopting unsophisticated pricing policies, (e.g., Mantovani et al., 2021). Finally, to avoid inconsistent standard errors, we cluster them on a higher level of aggregation, i.e., the hotel (see Angrist and Pischke, 2008).

Figure 1: Nights (in millions) spent in hotels (Eurostat).

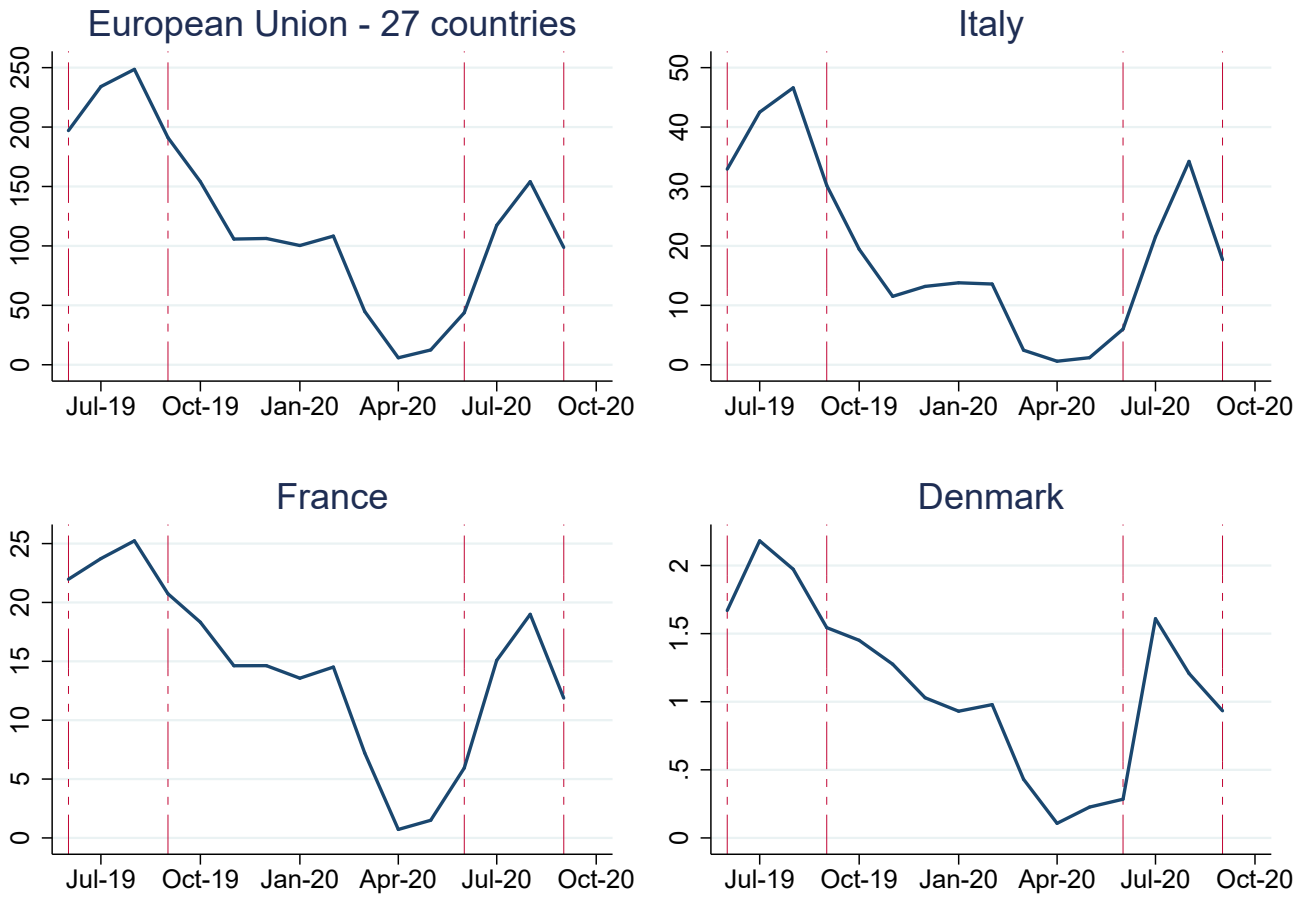
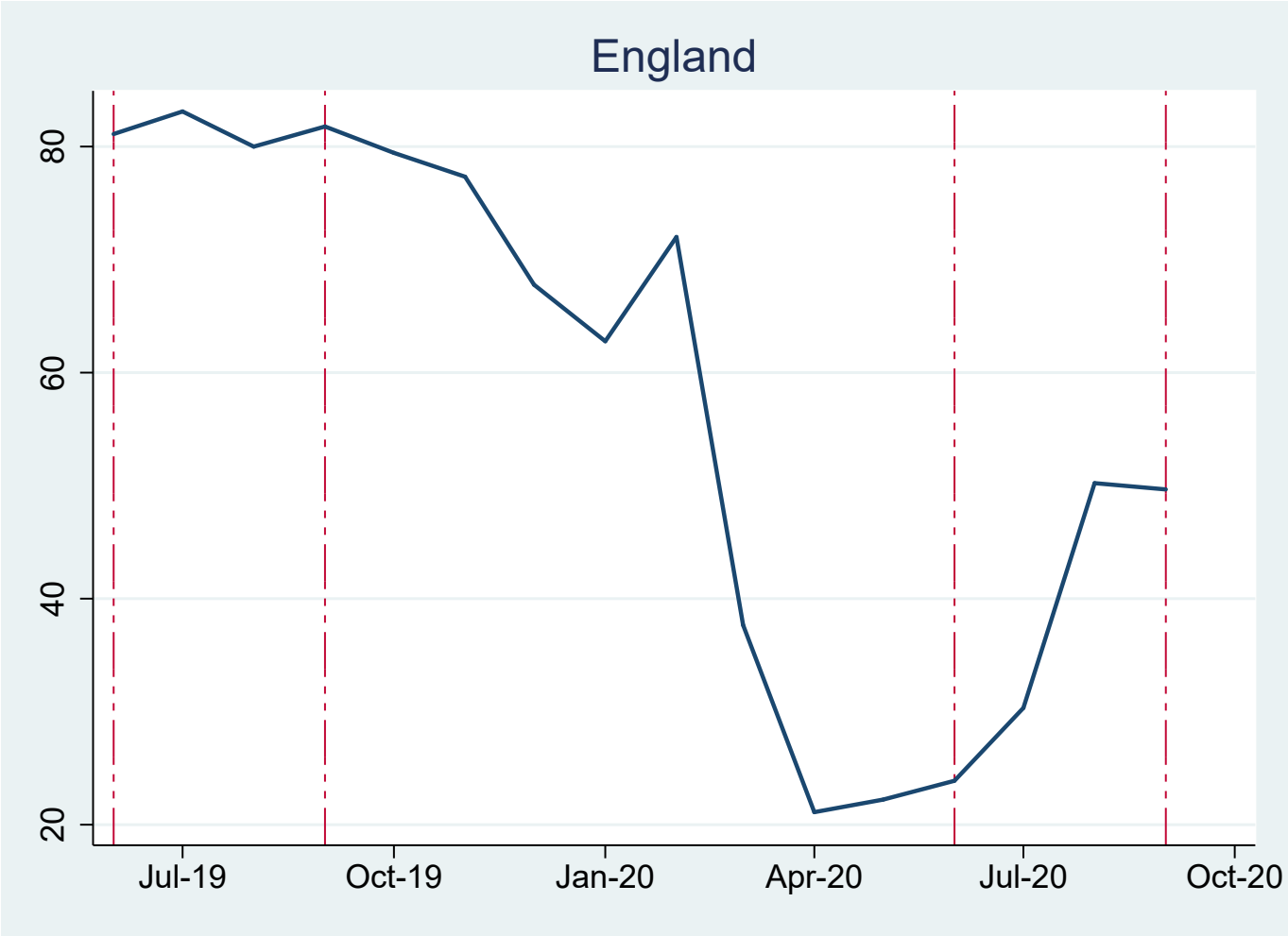


Figure 2: Occupancy rates in the accommodation businesses (ONS).



3 Results

In this section, we describe our main empirical results (from the DiD analysis) as well as the results of robustness tests, including the estimates obtained by using alternative methodologies (Nearest Neighbour Matching and Propensity Score Matching).

Baseline Regression. Table 1 reports the results from the estimation of equation (2) and is divided in four panels. Panel A refers to regressions run by using our main sample of products, while panels B, C, and D show the results obtained by adopting sub-samples of products located in UK and Denmark, UK and France, and UK and Italy, respectively. The table also presents distinct estimates for room variants referring to July and September check-ins.[14]

We begin by describing the estimates of our main parameters of interest, $\hat{\delta}_i$, i.e., the relative differences between the average percentage price variation before and after the policy introduction. Panel A shows relative price reductions between 2.8 and 3.5 percent, depending on hotel star classification, for products referring to July check-ins and observed in the first week after the policy change. The peak effect of the policy is reached in the second week after the reform for stays in July, where we estimate price reductions between 4.7 and 6.3 percent depending, as above, on hotel star classification. These results point to a quick but incomplete contemporaneous pass-through of the policy to the hotel prices, ranging between around 20 and 50 percent.[15] These findings are confirmed when using one control country at a time, i.e., Panel B Denmark, Panel C France and Panel D Italy.

The results reported in Table 1 also enable an evaluation of whether and how in July hotels adjusted the room prices for September check-ins. Noticeably, the estimated effects of the policy change are greatly reduced for prices observed in July for September stays. Specifically, Table 1 Panel A shows smaller $\hat{\delta}_i$'s, ranging from approximately -0.15 (3-star hotels) to -1.90 (5-star hotels) percent in the first and second week after the fiscal policy reform, respectively. These estimates are statistically significant only for 4-star and 5-star hotels. Qualitatively equivalent findings (with minor differences in terms of statistical significance for different hotel stars) are reported in panels B, C and D. Overall, they all point to a differential response to the policy in July by the hotel managers, who seem to have mainly focused on adjusting prices for July check-ins while keeping almost unaltered those for September stays. Taken

[14] We label the dummy variables $Post_1$, $Post_2$ and $Post_3$ as 'Post:15-21 July', 'Post:22-28 July', and 'Post:6-28 September'.

[15] With a decrease in the VAT rate from 20 to 5 percent, full pass-through corresponds to a fall in prices of 12.5 percent ($\frac{1.05-1.20}{1.20} * 100 = -12.5\%$). Therefore, a price reduction of 2.8 percent corresponds to a pass through of around 22 percent.

together, these results indicate that the VAT cut provided hotels with a strong discretion on whether to offer a discount on some of their services to their potential customers.

Table 1 also presents the estimates for the prices posted from the 6^{th} of September for September stays. In this case the effects of the policy on hotel room prices result more uncertain and often non-statistically significant, thus further confirming the transitory nature of the efficacy of the VAT cut. This result might be partly driven by the high uncertainty and drastic fall in hotel room prices in all countries under consideration, with reductions ranging between 10.5 and 16 percent (Post: 6-28 September, Panel A). Therefore, the threat of a second wave in Europe could have negatively affected the consumers propensity to travel, encouraging the hotels in all countries to charge lower prices.[16]

Overall, the results reported in Table 1 can be summarised as follows. For hotel prices posted in July for check-ins in the same month, we estimate a robust and statistically significant pass-through from the VAT cut that achieves its peak effect in the second week after the reform. Differently, the impact of the VAT cut on the UK hotel room prices for September check-ins was negligible, irrespective on whether these prices were observed in July or September. These results are robust to the use of different control groups and reveal the discretionary nature of the policy.

[16]The second wave of the pandemic started in France in mid-August, earlier than the other countries in our sample. The UK and Denmark started experiencing a surge in confirmed COVID-19 cases from the beginning of September, while Italy maintained low and stable numbers until the end of the same month (https://ourworldindata.org/coronavirus).

Table 1: The effects of the VAT reduction for different hotel stars and groups of countries.

	July Check-in			**September Check-in**		
	3*	4*	5*	3*	4*	5*
A. All Countries						
Post:15-21 July	−0.18	−0.17	−0.40	−0.94[a]	−0.83[a]	−0.51[b]
Post:22-28 July	−0.96[a]	−0.40	−0.45	−2.17[a]	−2.13[a]	−0.86[b]
Post:6-28 September	-	-	-	−16.1[a]	−13.4[a]	−10.5[a]
Post:15-21 July*Treated	−3.52[a]	−2.80[a]	−3.57[a]	−0.15	−1.35[b]	−1.79[a]
Post:22-28 July*Treated	−6.30[a]	−5.58[a]	−4.74[a]	−0.50	−1.63[b]	−1.90[a]
Post:6-28 September*Treated	-	-	-	3.76[a]	−2.22[b]	1.78
N	750,603	1,273,603	218,888	3,092,646	4,095,768	1,205,816
Adjusted R-sqr	0.52	0.50	0.51	0.65	0.62	0.59
Clusters	1,069	922	133	1,393	1,199	268
B. Control Country: Denmark						
Post:15-21 July	0.14	0.94[c]	−0.14	−0.07	0.21	−0.46
Post:22-28 July	0.10	1.81[c]	0.85	0.22	−0.69	−2.78
Post:6-28 September	-	-	-	−7.83[a]	−4.10[c]	−6.55[b]
Post:15-21 July*Treated	−3.18[a]	−3.14[a]	−3.19[b]	−1.03	−2.39[a]	−1.82[a]
Post:22-28 July*Treated	−5.91[a]	−6.08[a]	−4.58[b]	−2.88[b]	−3.06[a]	0.04
Post:6-28 September*Treated	-	-	-	−4.48	−11.5[a]	−1.83
N	215,326	359,049	114,291	736,871	1,350,433	720,995
Adjusted R-sqr	0.56	0.51	0.52	0.61	0.65	0.57
Clusters	207	232	50	268	336	136
C. Control Country: France						
Post:15-21 July	0.08	0.46[c]	0.76	−1.27[a]	−0.80[a]	−0.26
Post:22-28 July	−0.41	0.85[c]	0.93	−2.85[a]	−2.51[a]	0.14
Post:6-28 September	-	-	-	−20.1[a]	−17.9[a]	−9.02[a]
Post:15-21 July*Treated	−3.53[a]	−3.11[a]	−4.15[a]	0.21	−1.34[b]	−2.03[a]
Post:22-28 July*Treated	−6.32[a]	−6.10[a]	−4.79[a]	0.25	−1.17	−2.89[a]
Post:6-28 September*Treated	-	-	-	8.53[a]	3.09[b]	0.48
N	339,288	464,983	126,402	1,743,088	2,027,949	909,075
Adjusted R-sqr	0.54	0.51	0.53	0.69	0.67	0.59
Clusters	465	361	62	753	584	188
D. Control Country: Italy						
Post:15-21 July	−0.06	−0.17	−0.46	−0.74[a]	−0.97[a]	−0.70[c]
Post:22-28 July	−0.73[b]	−0.30	−0.28	−1.83[a]	−2.17[a]	−1.47[a]
Post:6-28 September	-	-	-	−12.8[a]	−12.0[a]	−11.9[a]
Post:15-21 July*Treated	−3.53[a]	−2.63[a]	−3.43[a]	−0.38	−1.20[b]	−1.58[b]
Post:22-28 July*Treated	−6.29[a]	−5.31[a]	−4.73[a]	−0.87	−1.59[b]	−1.27[c]
Post:6-28 September*Treated	-	-	-	0.11	−3.59[a]	3.48[c]
N	522,473	884,663	179,027	1,833,423	2,989,976	940,292
Adjusted R-sqr	0.52	0.51	0.51	0.60	0.62	0.59
Clusters	757	733	109	852	881	200

All the regressions are run by controlling for the variable T_d, i.e., the number of days (in logs) separating the observation of the price of a product and its check-in date.
Columns '3*', '4*' and '5*' show the results referring to 3-star, 4-star, and 5-star hotel prices, respectively.
Columns 'July Check-in' and 'September Check-in' report the results related to hotel prices for July stays and September stays, respectively.
Panels A, B, C, and D show the results obtained by using different control groups: prices of products located in Denmark, France and Italy (Panel A), Denmark (Panel B), France (Panel C), and Italy (Panel C), respectively.
The standard errors are clustered at the hotel level.
[a] p<0.01, [b] p<0.05, [c] p<0.1.

Controlling for Heterogeneous Effects. Table 2 shows the effects of the fiscal reform on the UK hotel prices controlling for different characteristics that might affect the hotels' reactivity to tax cuts. As explained in detail in Section 2, we take into consideration five types of bookings: 1) bookings with free cancellation; 2) bookings with breakfast included; 3) bookings in hotels with high management quality; 4) bookings in hotels with high overall customer satisfaction; and 5) bookings in hotels that are affiliated with a chain.

Focusing on the prices observed in July for stays in the same month, all DiD estimates from equation (3), $\hat{\delta}_i$, point to a robust and statistically significant pass-through from the VAT cut. Quantitatively, we find values in line with our baseline results.[17] As in the baseline regressions, the estimated policy effects decrease for prices observed in July for September check-ins, with smaller $\hat{\delta}_1$ that are statistically significant exclusively for 5-star hotels and certain types of bookings (i.e., bookings with free cancellation). In the second week after the reform, for stays in September, the estimates ($\hat{\delta}_2$) marginally increase relative to the first week, with statistically significant effects for room variants that include free cancellation or breakfast, in 4 and 5-star hotels. Looking at the prices observed after the 5$^{\text{th}}$ of September, we do not find evidence of strong policy effects, with DiD estimates that are volatile, with conflicting sign and often non-statistically significant. As explained above, this finding can be linked to the large fall in the hotel prices (Post: 6-28 September) in all countries examined here.

In summary, Table 2 shows that the main results presented in Table 1 apply also when we control for different observable characteristics of the product. We find statistically significant evidence of a relative reduction of the UK hotel prices, following the VAT cut, for room variants referring to July check-ins. However, this effect results transitory and the pass-through does not generally exceed 50 percent. Moreover, the coefficients of the triple interactions are generally small and non-statistically significant, indicating that hotels decided to spread the VAT cut uniformly across the range of products offered.[18]

[17] The only exception is the 85 percent pass-through ($\hat{\delta}_2 = -10.6$) for 5-star hotels, in second week after the reform, when controlling for free cancellation (Table 2, Panel A).

[18] The notable exception are the UK 5-star hotels, which appear to have significantly increased (by about 7 percent) the prices of room variants with the cancellation option, during the second week after the policy change (Table 2, Panel A).

Table 2: The effects of the VAT reduction for different hotel stars and type of booking.

	July Check-in			**September Check-in**		
	3*	4*	5*	3*	4*	5*
A. Booking with Free Cancellation						
Post:15-21 July	0.34	0.48[b]	1.41[b]	−0.83[a]	−0.68[a]	−0.27
Post:22-28 July	0.62[c]	1.84[a]	4.71[a]	−1.95[a]	−1.83[a]	−0.41
Post:6-28 September	-	-	-	−16.0[a]	−12.8[a]	−8.70[a]
Post:15-21 July*Treated	−3.67[a]	−3.56[a]	−5.70[a]	0.15	−1.12	−2.15[b]
Post:22-28 July*Treated	−6.36[a]	−6.31[a]	−10.6[a]	0.17	−2.25[b]	−2.58[a]
Post:6-28 September*Treated	-	-	-	4.99[a]	−2.25	0.90
Post:15-21 July*Free Cancellation	−0.68[a]	−0.85[a]	−2.25[a]	−0.17	−0.22	−0.30
Post:22-28 July*Free Cancellation	−2.22[a]	−2.98[a]	−6.35[a]	−0.36	−0.44	−0.58
Post:6-28 September*Free Cancellation	-	-	-	−0.20	−0.99	−2.21
Post:15-21 July*Treated*Free Cancellation	0.17	1.03	2.77	−0.51	−0.27	0.45
Post:22-28 July*Treated*Free Cancellation	−0.16	1.29	7.54[a]	−1.13[c]	0.85	0.85
Post:6-28 September*Treated*Free Cancellation	-	-	-	−2.04	0.21	1.22
B. Booking with Breakfast Included						
Post:15-21 July	−0.24	−0.33[c]	−0.28	−1.12[a]	−0.93[a]	−0.60[b]
Post:22-28 July	−1.05[a]	−0.67[b]	−0.23	−2.43[a]	−2.44[a]	−0.94[b]
Post:6-28 September	-	-	-	−17.6[a]	−14.7[a]	−10.8[a]
Post:15-21 July*Treated	−3.53[a]	−2.77[a]	−3.42[a]	0.51	−0.89	−1.41[b]
Post:22-28 July*Treated	−6.53[a]	−5.80[a]	−4.80[a]	0.14	−1.35[c]	−1.81[a]
Post:6-28 September*Treated	-	-	-	5.66[a]	−2.05[c]	0.88
Post:15-21 July*Breakfast	0.12	0.25[c]	−0.21	0.33[c]	0.16	1.16
Post:22-28 July*Breakfast	0.17	0.42	−0.39	0.51[c]	0.51[b]	0.14
Post:6-28 September*Breakfast	-	-	-	2.94[a]	2.03[a]	0.60
Post:15-21 July*Treated*Breakfast	0.03	−0.02	−0.30	−1.27[b]	−0.85[b]	−0.73
Post:22-28 July*Treated*Breakfast	0.49	0.43	0.07	−1.22	−0.46	−0.16
Post:6-28 September*Treated*Breakfast	-	-	-	−3.62[b]	−0.03	1.80
C. High Management Quality (HMQ)						
Post:15-21 July	−0.37	−0.34[c]	−0.25	−0.86[a]	−0.65[b]	−1.41[b]
Post:22-28 July	−1.15[a]	−0.82[b]	−0.16	−1.81[a]	−1.90[a]	−1.99[b]
Post:6-28 September	-	-	-	−14.5[a]	−12.2[a]	−12.5[a]
Post:15-21 July*Treated	−3.09[a]	−2.23[a]	−2.40[b]	−0.14	−0.57	0.29
Post:22-28 July*Treated	−5.28[a]	−5.23[a]	−4.73[a]	−0.03	−1.29	−0.59
Post: 6-28 September*Treated	-	-	-	1.28	−5.95[a]	−0.64
Post: 15-21 July*HMQ	0.38	0.31	0.23	−0.14	−0.28	1.17
Post: 22-28 July*HMQ	0.37	0.77	−0.45	−0.56	−0.36	1.45
Post: 6-28 September*HMQ	-	-	-	−2.67[b]	−2.03[c]	2.53
Post: 15-21 July*Treated*HMQ	−0.82	−1.10	−2.14	−0.03	−1.13	−2.59[b]
Post: 22-28 July*Treated*HMQ	−1.91	−0.65	−0.03	−0.75	−0.50	−1.69
Post: 6-28 September*Treated*HMQ	-	-	-	4.00	6.04[b]	3.12

Continued on next page

Table 2 – continued from previous page

	July Check-in			September Check-in		
	3*	4*	5*	3*	4*	5*
D. High Overall Satisfaction (HOS)						
Post:15-21 July	−0.40	−0.45[b]	−0.32	−1.26[a]	−0.66[b]	−1.33[c]
Post:22-28 July	−1.17[a]	−1.18[b]	−0.02	−2.17[a]	−1.78[a]	−1.72[c]
Post:6-28 September	-	-	-	−14.3[a]	−11.3[a]	−12.9[a]
Post:15-21 July*Treated	−3.02[a]	−2.34[a]	−3.38[a]	0.00	−1.01	−1.02
Post:22-28 July*Treated	−5.23[a]	−5.57[a]	−5.98[a]	−0.47	−1.87	−1.56
Post:6-28 September*Treated	-	-	-	2.26	−9.22[a]	−1.04
Post:15-21 July*HOS	0.35	0.42	−0.15	0.44	−0.24	1.07
Post:22-28 July*HOS	0.33	1.19[b]	−0.73	0.01	−0.48	1.09
Post:6-28 September*HOS	-	-	-	−2.52[c]	−3.07[b]	3.04
Post:15-21 July*Treated*HOS	−0.80	−0.74	−0.48	−0.20	−0.44	−1.04
Post:22-28 July*Treated*HOS	−1.72	0.01	2.51	−0.04	0.33	−0.51
Post:6-28 September*Treated*HOS	-	-	-	2.20	9.91[a]	3.60
E. Belonging to a Chain						
Post:15-21 July	−0.25	−0.18	0.52	−0.99[a]	−0.64[a]	−0.03
Post:22-28 July	−1.07[a]	−0.14	1.49[b]	−2.15[a]	−1.89[a]	−0.63
Post:6-28 September	-	-	-	−16.5[a]	−14.6[a]	−10.4[a]
Post:15-21 July *Treated	−3.66[a]	−2.06[c]	−2.03[c]	0.16	0.51	−1.17[c]
Post:22-28 July*Treated	−6.49[a]	−4.74[a]	−4.94[a]	0.09	−1.01	−1.19
Post:6-28 September*Treated	-	-	-	4.99[a]	0.79	2.49
Post:15-21 July *Chain	0.69	−0.03	−1.54[b]	0.48	−0.64[c]	−0.94[c]
Post:22-28 July*Chain	1.05	−0.83	−3.26[a]	−0.02	−0.72	−0.45
Post:6-28 September*Chain	-	-	-	3.32[b]	3.71[a]	−0.22
Post:15-21 July *Treated*Chain	0.46	−0.99	−2.43	−1.36	−2.37[b]	−0.70
Post:22-28 July*Treated*Chain	0.60	−0.72	0.40	−2.15	−0.53	−0.96
Post:6-28 September*Treated*Chain	-	-	-	−6.38[b]	−6.27[a]	−0.99
N	750,603	1,123,603	218,888	3,092,646	4,095,768	1,205,816
Adjusted R-sqr	0.52	0.50	0.52	0.65	0.62	0.60
Clusters	1,069	922	133	1,393	1,199	268

All the regressions are run by controlling for the variable T_d, i.e., the number of days (in logs) separating the observation of the price of a product and its check-in date.
Columns '3*', '4*' and '5*' show the results referring to 3-star, 4-star, and 5-star hotel prices, respectively.
Columns 'July Check-in' and 'September Check-in' report the results related to hotel prices for July stays and September stays, respectively.
Panels A, B, C, and D show the results obtained by controlling for different types of bookings: bookings with free cancellation (Panel A), bookings with breakfast included (Panel B), bookings in hotels with high management quality (Panel C), bookings in hotels with high overall customer satisfaction (Panel D), bookings in hotels that are affiliated with a chain (Panel E).
The standard errors are clustered at the hotel level.
[a] $p<0.01$, [b] $p<0.05$, [c] $p<0.1$.

Robustness checks. We run three main robustness checks. The first two robustness checks are aimed at controlling that the identifying assumptions of parallel trends and no-anticipation are satisfied. The third robustness check is an event study aimed at controlling whether the effect of the VAT cut grows by the amount of time that the policy has been in place.

First, we visually assess the common trends assumption. Specifically, we check whether the hotel prices moved uniformly before the reform in all the countries included in our analysis. In fact, since the VAT cut was announced on the 8$^{\text{th}}$ of July 2020 and implemented one week later, hotels in the UK could have started modifying their prices before the introduction of the tax cut. To do this, we regress the price index $\frac{P_v^d}{P_v^0}$ against the triple interaction of the dummy variable $Treated$, the categorical variable $Post_i$ and the check-in dates.[19] We then derive the predicted values of the price index for each check-in day and related query time, for the UK and control countries, separately. The resulting two series of estimated prices, by hotel stars, are shown in Figure 3.[20]

For each star classification, Figure 3 presents four graphs with predicted price indices depending on when prices were retrieved, i.e., between the 9$^{\text{th}}$ and the 14$^{\text{th}}$ of July (top-left quadrant), between the 15$^{\text{th}}$ and 21$^{\text{st}}$ of July (top-right quadrant), between the 22$^{\text{nd}}$ and 28$^{\text{th}}$ of July (bottom-left quadrant), or after September the 5$^{\text{th}}$ (bottom-right quadrant), respectively. The series of estimated prices in the top-left quadrant of each panel, i.e., (a), (b) and (c), do not exhibit any significant discrepancy. They are both strongly centred around the value of 1, meaning that hotels in the UK did not lower their prices before the reform was implemented. These results suggest that the parallel trends and no-anticipation conditions hold.

Moreover, to further provide robustness against any potential violation of the parallel trends assumption, we repeat the baseline treatment effect analysis by adopting the Nearest Neighbour Matching (NNM) and the Propensity Score Matching (PSM). To the purpose of these techniques, we selected homogeneous clusters of room variants, characterised by the same check-in period, query time, and hotel star classification. For both methodologies, we calculate distance and score by using the following variables: number of days separating the query from the check-in date, check-in date, chain affiliation, overall customer satisfaction, and location rating. Furthermore, for the NNM approach, we also have exact matching for chain affiliation and check-in date.

Results from adopting the NNM and PSM approaches are reported in Table 3 and are

[19] All the regressions are run by controlling for the variable T_d.

[20] A similar graphical analysis of common trends, using predicted hotel prices, has been recently implemented by Mantovani et al. (2021).

consistent with those presented in Tables 1 and 2 for prices referring to July check-ins. Specifically, the NNM estimates range between -2.1 and -3.9 percent in the first week and -5.8 and -6.7 percent in the second week after the fiscal policy reform. The implied estimated pass-through is between around 17 and 31 percent in the first week and between 46 and 54 percent in the second week. Similarly, the PSM estimates show price reductions between -2.6 and -4.8 percent in the first week and -5.8 and -7.6 percent in the second week after the VAT cut, thus indicating a slightly higher pass-through. Differently, for the hotel prices for September check-ins, the Average Treatment Effect on Treated (ATT) estimates obtained with NNM and PSM present conflicting results in terms of the impact of the policy, with positive effects for products offered by 3-star hotels and negative effects for room variants located in 4-star and 5-star hotels. These estimates, however, further corroborate the interpretation that hotels in July adjusted prices differently depending on the check-in period. Indeed, as shown in Table 3, the prices set in July for stays in September reduce much less (and sometimes increase) relative to prices posted in July for stays in the same month.

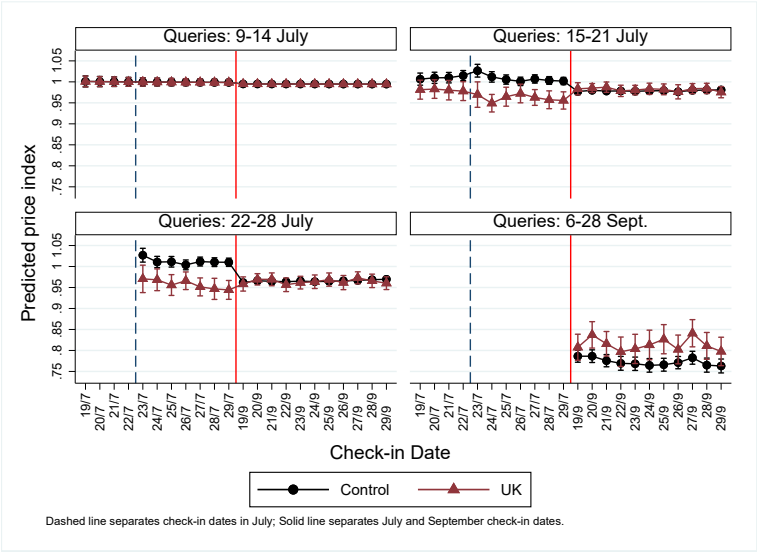
Our last robustness check consists in conducting an event study aimed at further checking whether the effect of the VAT cut grows by the amount of time that the policy has been in place. If this turned out not to be the case, it would violate the assumption that the effect of the policy change is the same across hotels but not across time, thus invalidating our main DiD identification. Table 4 shows the dynamic effects of the policy obtained by adopting two methods based on the Abadie (2005)'s Inverse Probability Weighting (IPW) DiD approach and the more recent Doubly Robust (DR) DiD technique (Callaway and Sant'Anna, 2021), respectively. For both methodologies, we calculate the ATTs on any specific query time (before and after the VAT cut). For the purpose of this analysis, we only use prices observed in July for July and September check-ins, separately. The never-treated hotels are the comparison group.

Table 4 reports that all the pre-treatment ATTs (Pre: 9-14 July) are small and non-statistically significant. Differently, the post-treatment estimates referring to the overall period 15-28 July show significant price reductions (-3.6 and -4.5 percent, respectively) for products related to July check-ins. Focusing on the daily estimates, the effect of the tax cut on the UK hotel prices appears to be negative and increasing in magnitude the longer the hotels are exposed to the fiscal reform. For example, according to the DR estimates, the prices of the UK hotels are estimated to be 1 percent lower on average on the 15^{th} of July than they would have been in the absence of the VAT cut. On the 20^{th} of July they are estimated to be 2.5 percent lower; on the 25^{th} of July, 5.3 percent lower; and on the 28^{th} of July, 6.6 percent lower. The IPW ATTs describe similar dynamics. The post-treatment estimates for prices posted during the period 15-28 July for September stays are, on the

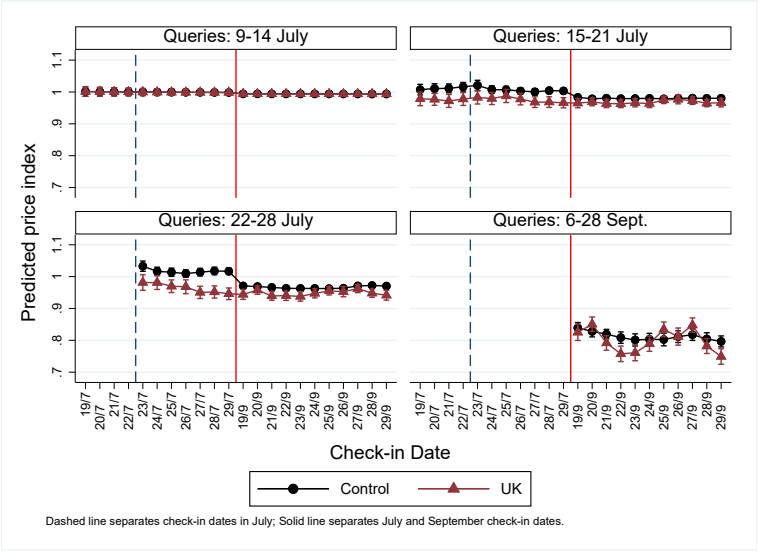
contrary, small and non-significant (at 5 percent level of significance), and provide further corroboration of a discretionary approach by British hotel managers, that in July did not alter September prices in response to the VAT cut. Therefore, the event study confirms that, in line with the empirical results reported in Tables 1 and 2, the effects of the policy change on the UK hotel prices observed in July for stays in the same month varied with the length of exposure to the fiscal reform and achieved its peak in the second week after the reform, but only for a subset of products.

Figure 3: Predicted prices by query and check-in dates.

(a) 3-star hotels



(b) 4-star hotels



(c) 5-star hotels

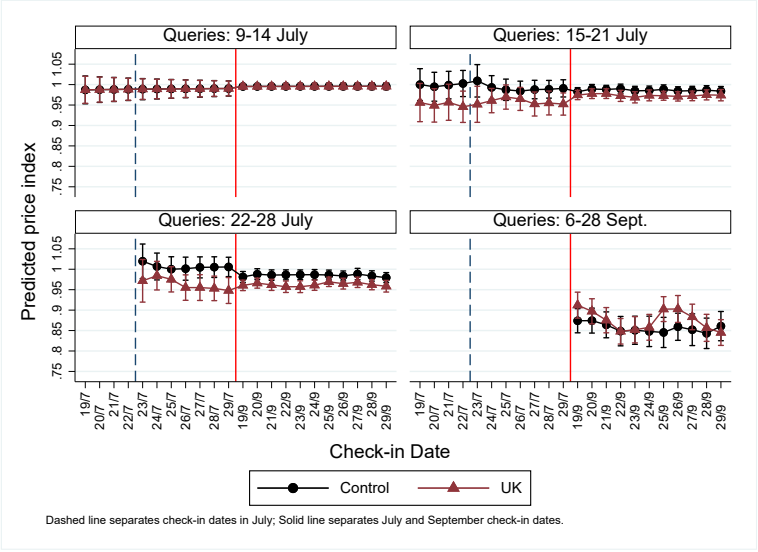


Table 3: Nearest Neighbor Matching and Propensity Score Matching.

	Nearest Neighbor Matching ATT◊					
	July Check-in			**September Check-in**		
	3*	4*	5*	3*	4*	5*
Post: 15-21 July	-3.88^{a}	-2.08^{a}	-3.65^{a}	1.77^{a}	-1.33^{a}	-3.87^{a}
Post: 22-28 July	-6.72^{a}	-5.80^{a}	-5.61^{a}	3.80^{a}	-1.31^{a}	-3.92^{a}
Post: 6-28 September	-	-	-	4.57^{a}	-1.61^{a}	-1.37^{a}
	Propensity Score Matching ATT					
	July Check-in			**September Check-in**		
	3*	4*	5*	3*	4*	5*
Post: 15-21 July	-4.61^{a}	-2.60^{a}	-4.81^{a}	1.62^{a}	-1.51^{a}	-3.63^{a}
Post: 22-28 July	-7.56^{a}	-5.84^{a}	-7.05^{a}	0.05^{a}	-1.13^{a}	-4.68^{a}
Post: 6-28 September	-	-	-	2.70^{a}	-0.18^{a}	-1.64^{a}

Columns '3*', '4*' and '5*' show the results referring to 3-star, 4-star, and 5-star hotel prices, respectively. Columns 'July Check-in' and 'September Check-in' report the results related to hotel prices for July stays and September stays, respectively.

For July check-in dates, the sample sizes in the first row (week 1) of estimates are $362,617$; $548,653$ and $104,810$; in the second row (week 2) $141,684$; $209,175$ and $38,332$. For September check-in dates, the sample sizes in the first row (week 1) of estimates are $891,978$; $1,181,958$ and $359,857$; in the second row (week 2) $955,969$; $1.227,148$ and $359,206$; in the third row (September) $797,761$; $1,078,689$ and $256,529$.

◊ Exact matching on check-in dates and chain affiliation.

[a] $p<0.01$, [b] $p<0.05$, [c] $p<0.1$.

Table 4: Event Study: Dynamic Effects.

	Doubly Robust DiD		**Inverse Probability Weighting DiD**	
	July Check-in	**September Check-in**	**July Check-in**	**September Check-in**
Pre: 9-14 July	−0.19	−0.09	−0.22	−0.09
Post: 15-28 July	−3.61[a]	−0.33	−4.52[a]	−0.73[c]
Post: 15 July	−0.99[a]	−0.55[b]	−1.06[a]	−0.56[b]
Post: 16 July	−1.98[a]	−1.08[b]	−2.23[a]	−1.15[b]
Post: 17 July	−1.61[a]	−0.50	−1.76[a]	−0.78[c]
Post: 18 July	−2.26[a]	−0.43	−2.56[a]	−0.70
Post: 19 July	−2.36[a]	−0.29	−3.17[a]	−0.74
Post: 20 July	−2.51[a]	−0.10	−3.20[a]	−0.18
Post: 21 July	−2.78[a]	0.04	−3.23[a]	−0.04
Post: 22 July	−3.44[a]	−0.36	−4.05[a]	−0.34
Post: 23 July	−4.14[a]	−0.33	−5.44[a]	−0.67
Post: 24 July	−4.90[a]	−0.31	−6.43[a]	−0.80
Post: 25 July	−5.33[a]	−0.28	−6.72[a]	−1.76[c]
Post: 26 July	−5.71[a]	−0.31	−7.08[a]	−1.68[c]
Post: 27 July	−6.03[a]	0.07	−7.17[a]	−0.15
Post: 28 July	−6.55[a]	−0.20	−9.25[a]	−0.61
N	1,959,714	6,127,236	1,984,215	6,201,390
Clusters	2,096	2,833	2,096	2,833

All the regressions are run by controlling for the variable T_d, i.e., the number of days (in logs) separating the observation of the price of a product and its check-in date.
Columns 'July Check-in' and 'September Check-in' report the results related to hotel prices for July stays and September stays, respectively.
The standard errors are clustered at the hotel level.
[a] $p<0.01$, [b] $p<0.05$, [c] $p<0.1$.

4 Conclusions

On the 15th of July 2020, the UK Government introduced a temporary VAT cut in the hospitality sector from the base rate of 20 to 5 percent. In this paper, we examine whether this tax cut was passed on to consumers via hotel prices reductions. In conducting our analysis, we adopt a quasi-experimental approach and use a novel dataset comprising 745,892 room variants over 3,038 hotels.

We describe our findings as follows. For hotel prices posted in July for check-ins in the same month, we estimate a robust and statistically significant pass-through from the VAT cut that varies between around 20 and 50 percent. Furthermore, we find that its peak effect happens in the second week after the reform, for July check-ins. On the contrary, the impact of the tax cut on the UK hotel room prices for September check-ins was almost negligible both in July, when the outlook on the end of the pandemic was more optimistic, and in September, when the prospect of another lockdown became more likely. Taken together, the core message of our study is that the effect of the targeted VAT policy on consumer prices of hotel rooms, although substantial, was incomplete in magnitude and limited to certain products because hotel managers could apply strong discretion on whether to pass discounts on to customers. These conclusions are robust to the use of different control groups, the inclusion of several heterogeneous effects in the econometrics model, controlling for parallel trends and no-anticipation, and alternative estimation techniques.

All combined, the insights from this study may help governments in their implementation of measures aimed at supporting the economy during economic downturns or periods of high inflation. For example, to contain the inflationary pressures partly induced by the recent energy crisis many governments are indeed considering VAT cuts for important daily grocery items, e.g., food like pasta, bread, vegetables, etc. Our results cast serious doubts on this type of policies, as these reforms may act mainly as supply-side subsidies, e.g., Benzarti and Carloni (2019), with only a minor fraction of potential benefits discretionally passed on to consumers.

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Online Appendix

A Tourism data

Figure A.1: Examples of prices posted on a London hotel's page